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THE JOURNAL OF PHILOSOPHY

ENTHYMEMES*

I

THERE is a classical category of arguments, discussions of which enjoyed a certain vogue toward the end of (what Bochenski has called) the Dark Ages in logic (i.e., from the Renaissance up until near the turn of the present century), namely, *enthymemes*. We take our text from Jevons 1870[1] (pp. 153–154):

> A syllogism when incompletely stated is usually called an *enthymeme*, and this name is often supposed to be derived from two Greek words (εν, in, and θυμός, mind), so as to signify that some knowledge is held by the mind and is supplied in the form of a *tacit*, that is a silent or understood premise. . . . Of this nature is the following argument: 'Comets must be subject to the law of gravitation; for this is true of all bodies which move in elliptic orbits.' It is so clearly implied that comets move in elliptic orbits, that it would be tedious to state this as the minor premise in a complete syllogism of the mood Barbara, thus:
>
> [Major premise (M)] All bodies moving in elliptic orbits are subject to the law of gravitation;
>
> [Minor premise (m)] Comets move in elliptic orbits;
>
> [Conclusion (C)] Therefore comets are subject to the law of gravitation.

Now everyone agrees that the syllogism as stated in full is valid and that a definition of "validity" that denied validity to the syllogism in Barbara would *ipso facto* be faulty. It is also conceded that corresponding to this form of inference there exists a certain *true* proposition,[2] namely,

If M and m, then C.

The case of the enthymematic inference and the corresponding proposition

If M, then C

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[1] Dates refer to the bibliography at the end of the paper.

[2] Or propositional form; such distinctions are irrelevant to our purposes, and will be observed only where essential (which in this paper they never are).

may, however, occasion some doubt. Asked whether the enthymematic inference is valid (or whether the proposition just above is true), we may want to answer either, "No, your premises are simply insufficient for your conclusion," or, "Yes, provided you mean to be using the obviously required premise m (which we grant that you are, we being in a tolerant mood)."

Now, whichever attitude we may wish to take, it is clear that there is a difference: in the first case there is a robust relation of logical consequence between premises and conclusion; in the second the *logical* connection is largely, if not entirely, by courtesy. And notice that no one has any difficulty in seeing the clear difference. So much, at any rate, we wish to take for granted; and we now turn to our topic, which is what to make of the situation. In particular, what sort of formal analysis of *if* ... *then*___ and *and* can do justice to the clearly felt distinction considered above?

For present purposes we wish to treat *and* in a purely truth-functional way: M *and* m is true if and only if M is true and also m is true.

We now ask whether the true *if* ... *then*___, corresponding to the syllogism, should be represented by the material "implication":

$$M \,\&\, m \supset C.$$

We have argued elsewhere at length (and somewhat peevishly) that material "implication" is simply *not* a kind of implication (Anderson and Belnap, 1961a, 1961b). We do not plan to repeat those arguments here; rather we shall offer some fresh arguments for the same thesis. Our point, incidentally, is also telling as against the view that intuitionistic "implication" is a relation of entailment, or logical deducibility.

The present argument is simply this: both views make hash of the distinction between logically valid arguments and enthymemes. For on both theories we have as a "theorem of logic" a principle according to which true premises aren't really there at all ("true premises may be suppressed"):

$$\frac{\begin{array}{c} m \\ M \,\&\, m \supset C \end{array}}{M \supset C.}$$

Suppose now that an argument is valid if and only if the corresponding material or intuitionistic "implication" statement is true. Then, since the argument from M and m to C is valid, $M \,\&\, m \supset C$ is true; and since m is true as well and, hence, suppressible, $M \supset C$ is true; hence the argument from M to C is valid, and *in precisely the same sense* as is the argument from M and m to C. But to say that the

argument from M to C is thus valid is in direct contradiction to the doctrine that enthymematic arguments suppress *required* premises. "Where a necessary premiss is missing, without that premiss the inference is invalid" (Copi 1953, p. 205).

The conclusion seems inescapable that one cannot *both* hold that material and intuitionistic "implication" relations answer to the notion of a valid argument *and* try to maintain a distinction between valid arguments and enthymemes.

Now it is not unmistakably clear whether or not anyone ever seriously took material implication to explicate "valid argument," but we have no doubt on this score in respect to Lewis's theory of strict implication. We are told that the meaning of strict implication "is precisely that of ordinary inference and proof" (Lewis, 1912, p. 531; see also Lewis and Langford, 1932, p. 247).

And in the systems of strict implication, one *can* maintain a distinction between valid arguments and enthymemes, at least those of the sort we have considered above. In particular, if we put hooks for horseshoes, a conjoined premise may not be dropped simply on the grounds that it is true. "That is, in stating a strict implication one cannot omit a merely true premise which is one of a set of premises which together give the conclusion" (Lewis and Langford, 1932, p. 165).

So far so good.

II

Now consider the following argument:

> Copi, in the passage quoted above, is *clearly* correct in holding that valid arguments may be distinguished from enthymemes and that enthymemes as they stand (i.e., without statement of the required supporting premiss) are not valid. Therefore, if all bodies moving in elliptic orbits are subject to the law of gravitation and if comets move in elliptic orbits, then comets are subject to the law of gravitation.

The reader may find the reasoning in the foregoing paragraph somewhat intricate and difficult to follow, so we forthwith lay bare its logical form. Where A represents "enthymemes may be distinguished from valid arguments" and B represents "enthymemes as they stand are not valid," the argument has the form:

$$\frac{A \text{ and } B}{\text{If } M \text{ and } m, \text{ then } C.}$$

When one is asked whether this argument is valid, the initial reaction is likely to be "No," perhaps because the premises A and B have nothing to do with the conclusion: if M and m then C. But there have been sophisticated logicians, Lewis for example, who, on

reflection, decided that the argument *is* valid, on the grounds that the consequent, if M and m then C, is necessary. But this view is debatable (notoriously), and the notion that a necessary proposition is entailed by *any* proposition has been heatedly and repeatedly attacked. That is, there have been those who (1) have held that the lack of relevance between premise and conclusion, just above, reduces to absurdity that claim that strict implication captures intuitive *entailment*, and those who (2) have defended strict implication on the grounds that the well-known paradoxes do indeed represent "facts about deducibility." We shall want ultimately to side with the first group; the ludicrous argument above cannot really be regarded as valid. But sophisticated logicians must have *something* in mind, and we shall try later to give such devils their due. Still, the argument above is no more valid as it stands than is the move from "all bodies moving in elliptic orbits are subject to the law of gravitation" to "comets must be subject to the law of gravitation," i.e., the move from M to C.

But now an analogy suggests itself. Couldn't we try to view the odd argument cited above as somehow enthymematic and by this irenic maneuver pacify both parties to the dispute? For *everyone can see* that

$$\frac{A \text{ and } B \text{ and } (\text{if } M \text{ and } m \text{ then } C)}{\text{If } M \text{ and } m, \text{ then } C}$$

is valid, and that

$$\frac{A \text{ and } B}{\text{If } M \text{ and } m, \text{ then } C}$$

arises from it by suppressing a true (indeed necessarily true) premise. And *everyone can see* that there is a difference between these two cases; else there could have been no sensible dispute in the first place.

Of course these latter enthymemes ("strict enthymemes") differ from those of section I (which, for reasons to be brought up later, we shall call "intuitionistic enthymemes"[3]) in that the suppressed premise is *necessarily* true, rather than simply *true*. But the suppression of necessary premises is, if anything, less objectionable than the suppression of contingently true ones; necessarily true premises are suppressed constantly in arguments in mathematics and in logic, simply because one does not want (ordinarily) to

[3] We have considered, and rejected, the following harrowing course: to retain "enthymeme" in its customary usage, and introduce "ananceënthymeme" for what we here call "strict enthymemes." (The term was supplied, at our insistent request, and with understandable reluctance, by a colleague in the department of classics, who asked us not to mention his name. It was Ralph L. Ward.)

bore one's audience with endless repetition of obvious logical truths (as *we* have been doing).

At any rate we shall take it, for the present, that the *sense* behind the claim that a necessary proposition is entailed by *any* old proposition is that, in the alleged entailment, a necessary premise is suppressed: to wit, the conclusion. And we offer this view as face-saver for both parties. To take another example, the statement "If *A*, then either *A*-and-*B* or *A*-and-not-*B*" can be regarded as *true*, all right; we have just left out an obvious, and obviously required, premise: "*B* or not-*B*."

But if we are asked whether strict implication can be taken as an account of entailment, or logical deducibility, the answer must again, as in the case of material and intuitionistic "implication," be "No."[4] For as Lewis himself points out, strict implication is enthymematic in the sense we have just described; necessarily true premises may be suppressed. "The omission of a premise which is *a priori* or logically undeniable does not affect the validity of deduction" (Lewis and Langford, 1932, p. 165). How *could* this be so? Can we make *no* distinction between valid and invalid arguments when, as may happen, all the propositions in the argument are necessary? We believe, rather, that the situation is as the citation above from Copi may be misinterpreted as saying: "Where a necessary premise is missing, without that premise the inference is invalid." Necessary premises are just as necessary as premises that are not necessary.

III

In the first two sections we have thrown the word "entailment" about in a way that suggested we knew what it meant. Well, we claim to. And, in particular, we claim that the formal system E of entailment accurately captures the intuitive concept of entailment, or logical deducibility. The literature on the system E is burgeoning,[5] and neither time nor space permit anything like a full account of what is known about it. But we here list briefly a few of its more important features, by way of preparing for a formal account of enthymemes in the next section.

Roughly, the system E is just like Lewis's S4, except that paradoxical assertions are missing. Such principles as $A \,\&\, \bar{A} \rightarrow B$, $A \rightarrow B \vee \bar{B}$, and $NA \rightarrow . B \rightarrow A$ [where NA means A is necessary, defined as $(A \rightarrow A) \rightarrow A$], are not provable in E (where the arrow

[4] We note that exactly the same argument can be made against the usual semantical definition of "validity" for the first-order functional calculus—if, that is, the horseshoe is thought of as an *if . . . then _ _ _* relation.

[5] See items by Anderson, Belnap, and Wallace, in the bibliography.

is the symbol for entailment). But any *sensible* kind of inference is allowed. The first two properties listed below show some ways in which E is weaker than strict implication; the third through sixth show that, in spite of weaknesses, the system is strong.

1. As a minimal requirement for relevance of A to B (in propositional logic), it seems reasonable to demand that if $A \rightarrow B$ is accepted, then A and B should share a propositional variable. E satisfies this condition (Belnap 1960b).

2. Entailments are *sui generis* in the following sense: only entailments entail entailments. More sharply: if $A \rightarrow . B \rightarrow C$ is provable in E, then A contains an arrow (Ackermann 1956). Moreover, it is never the case that the *denial* of an entailment entails an entailment; i.e., no theorems have the form $\overline{A \rightarrow B} \rightarrow . C \rightarrow D$.

So the arrow is weaker than either the horseshoe or the hook. On the other hand, a decent theory of entailment, while avoiding fallacies of modality and of relevance, should not throw *all* the babies out with the bathwater. A satisfactory theory will isolate the disease of raw irrelevance, keeping everything of the standard theories that can possibly be kept:

3. E contains the full two-valued propositional calculus (Ackermann 1956), except of course that $\bar{A} \vee B$ is not to be thought of as an implication relation.

4. Every truth-functional compound coentails a disjunctive normal form and a conjunctive normal form, which means that, for conjunction and disjunction, the usual associative, commutative, and distributive rules hold and that we have rules of double negation and De Morgan's laws. More generally, if $A_1 \vee \cdots \vee A_m$ is a disjunctive normal form of a truth-functional A and if $B_1 \,\&\, \cdots \,\&\, B_n$ is a conjunctive normal form of a truth-functional B, then A entails B if and only if each A_i entails each B_j; i.e., if and only if each A_i and each B_j share a (negated or unnegated) variable (Belnap, 1959; Anderson and Belnap, 1961b).

5. Generalization to quantifiers is easy, and the extensional fragment of the system (already known in the literature as "the first-order functional calculus") is complete, as can be seen by a rather trivial proof (after Schütte 1953).

6. The semantics of first-degree entailments (i.e., entailments $A \rightarrow B$, where A and B contain only truth functions and quantifiers) is completely understood (Anderson and Belnap, 1961c), though there are many open questions about the semantics of more complicated formulas.

Such are, in rough outline, some of the principal features of the system E of entailment, which embodies the intuitive concept of

entailment, or *logical consequence* (or so we claim). Further discussion of it will have to await another time and place; we turn now to a reconsideration of enthymemes within the context of the system E.

IV

Now what has been required, classically, of "valid" enthymemes? Two things: (1) that it be obvious what the suppressed premise is; (2) that the suppressed premise be obviously true. Degrees of obviousness are of course psychological matters, which we dare not touch. But when the psychological requirements of obviousness are stripped away, we are left with a consideration that logic should be able to handle: the suppressed premise must be *true*.

This leads us to suggest that if we are right in maintaining that E correctly captures the intuitive notion of a valid argument, then (allowing ourselves propositional quantifiers) the following would be a reasonable definition of an enthymematic *if* . . . *then*_ _ _ (which we symbolize by $\rightarrow^{\bullet}$):

$$A \rightarrow^{\bullet} B =_{df} (\exists r)[r \,\&\, (r \,\&\, A \rightarrow B)].$$

That is, we want to say that the *if* . . . *then*_ _ _ proposition corresponding to any enthymematic argument is true if and only if there is a (suppressed) premise which is true and which would convert the enthymeme into a valid argument. And when no more than *contingent* truth of the suppressed premise is required, we have enthymemes of the sort previously referred to as "intuitionistic." We now justify that terminology.

That intuitionistic "implication" is a relation of logical implication, or deducibility, has not been maintained even by some of its strongest proponents. Heyting 1956, for example, writes:

> The *implication* $p \rightarrow q$ can be asserted, if and only if we possess a construction r, which, joined to any construction proving p (supposing that the latter be effected), would automatically effect a construction proving q. In other words, a proof of p, together with r, would form a proof of q (p. 98).[6]

One could hardly ask for a clearer or more explicit statement to the effect that intuitionistic "implication" is *enthymematic*; i.e., when $p \rightarrow q$ is intuitionistically true, there is a true premiss r, *which may be required* for the construction of q from p, but which is omitted in the antecedent p.

Now apparently Heyting has in mind a distinction between the sense in which p intuitionistically "implies" q (in the presence of r)

[6] As another example we mention an interesting paper of Curry 1959; he says "The absoluteness of absolute [i.e., intuitionistic] implication does not depend on any claim to its being a definition of logical consequence. It does not pretend to be anything of the sort" (p. 20).

and some other sense of *if . . . then_ _ _* in which *if p and r then q* is true. Nothing is said about the latter relation, nor indeed can the intuitionistic formalism reflect the intuitive distinction between "implies" and "would automatically effect a construction." But if we are correct in claiming that the definition above (of $\rightarrow^{\bullet}$) captures the notion of an enthymematic *if . . . then_ _ _* and if Heyting is correct in saying that intuitionistic "implication" is enthymematic then we should expect that intuitionistic "implication" and our enthymematic "implication" should exactly coincide.

And this turns out to be the case. It develops that if we add propositional quantifiers to the positive fragment of E (i.e., if we consider that fragment EP^{+} got from E by omitting those axioms involving the negation sign, and adding propositional quantifiers) and if we further define

$$A \supset B =_{df} (\exists r)[r \,\&\, (r \,\&\, A \rightarrow B)],$$

then the set of all theorems of EP^{+} that contain only $\supset$, &, and $\vee$ *coincides exactly* with the positive fragment of intuitionistic logic as formalized by Heyting. That is, "A intuitionistically 'implies' B" *means* that there is some true proposition r such that the conjunction of r and A *entails* B.[7]

So we may justifiably (and in conformity with Heyting's own intuitive discussion) think of the intuitionistic horseshoe as expressing a notion of enthymematic (intuitionistically enthymematic) "implication": the antecedent doesn't *really* guarantee the truth of the consequent—but there *are* some true propositions which, together with the antecedent, could really be shown to have the consequent as a logical consequence.

There has been abroad for a long time a notion to the effect that entailment is a "simply psychological" matter[8]—and that the only difference between (say) the law of transitivity and the "law" $A \supset . B \supset A$ is that one has a nice, warm psychological feeling in the presence of the former and a somewhat sick psychological feeling in the presence of the latter. We think that the result just stated justifies a charge of psychologism in the other direction. The *only* reason anyone could ever have had for thinking that we could justifiably omit required premises that happened to be true is that

[7] See Myhill, 1953, which suggested this idea to us.

[8] See again Curry, 1959: ". . . it goes without saying that absolute implication is an objective concept in precisely the same sense that material implication is. It does not depend on any subjective feelings of 'entailment' " (p. 21). Similarly, "These methods [those of McKinsey] are capable of taking some of the mysticism out of the idea of necessity" (p. 23). We contend that the only mysticism in the idea of logical necessity is such as has been *put* there, largely by Quine, Goodman, White, and their followers.

we all knew that they were true, and the fact just didn't need to be mentioned (in the social context of the argument). At least it is hard to see any other reason why anyone would think (harking back to an earlier example) that the move from M to C was justified at all.

If, moreover, we define intuitionistic negation as follows:

$$\neg A =_{df} A \supset (p)p,$$

then the set of theorems of EP$^+$ containing only $\supset$, $\neg$, &, and $\vee$ *coincides exactly* with the full system of intuitionistic propositional calculus. That is, "A is intuitionistically false" *means* that A intuitionistically "implies" that every proposition is true.

We take these facts as evidence that when people have talked of the horseshoe as a relation of "implication" or "entailment" or "deducibility" or any of a host of equivalents, they have *really* had in mind (at best) a relation of (intuitionistically) *enthymematic* "implication," "entailment," "deducibility," etc.

Turning now from intuitionistic enthymemes to strict enthymemes, we observe that the appropriate definition (in EP$^+$) of a strictly enthymematic *if* . . . *then*_ _ _ would be

$$A \dashv B =_{df} (\exists r)[Nr \,\&\, (r \,\&\, A \rightarrow B)] \qquad \text{(where N is necessity).}$$

And it turns out that the set of theorems of EP$^+$ that contain only $\dashv$, &, and $\vee$ *exactly coincides* with the positive fragment of Lewis's system S4 of strict implication.[9] That is, "A strictly 'implies' B" (in the sense of the positive part of S4) *means* that there is some *necessarily* true proposition r such that the conjunction of r and A *entails* B. Again, the intuitive considerations of our second section are borne out: if we consider only *strict* enthymemes, in which the discarded premise must be necessary, then we get precisely the (positive) system advocated by the advocates of S4.

V

We claim in this paper to have established (or commented on the establishability of) the following (obvious) truths:

1. A distinction can be drawn between valid arguments and enthymemes, and between enthymemes of two sorts: strict and intuitionistic. This is so trivial a point that it hardly seems worth arguing for. It *does* need arguing for, but that is only because currently accepted analyses of *implication* are so goofy.

2. Neither the horseshoe nor the hook will do as an analysis of the notion of a *valid argument* (most elementary and advanced texts to the contrary notwithstanding), since neither theory can distinguish between valid arguments and strict enthymemes.

[9] In a formulation where the primitives are $\dashv$, &, and $\vee$.

3. The formal system E of entailment *does* suffice as an explication of the intuitive notion of entailment, or formal deducibility, and *does* account for the differences among valid arguments, strict enthymemes, and intuitionistic enthymemes.

4. What are we to think about enthymematic arguments? A priori, one might say that the notion of an enthymematic argument is sociological (depending on what we can expect our hearers to know) or psychological (depending on what *we* claim to know) and therefore is *a*logical: it is not a part of logic to try to take account of who knows what; and anyway our intuitions concerning enthymemes are altogether too unstable to support any kind of mathematical or logical theory. We on the contrary maintain that there is nothing intrinsically *bad* about sociology and psychology, even from a logician's point of view. Moreover, one can give a precise interpretation to the (admittedly, at the outset, somewhat *vague*) notion of an enthymematic argument. Thus:

5. If we are sufficiently careless in stating our arguments so that we allow ourselves to suppress any true premise, then we arrive *precisely* at the intuitionistic theory of "implication."

6. If we are somewhat more careful, and allow ourselves to suppress only necessarily true premises, then we arrive *precisely* at strict "implication" (in the sense of the positive fragment of S4).

7. If we are *very* careful, and always put down all the premises we need (i.e., if we argue *logically*), then we arrive *precisely* at the formal system E of logical implication (without quotes, this time), or *entailment*.

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MODALITIES AND EXTENDED SYSTEMS *

MODAL words are used in many different situations, and it is not clear which of them various modal logics are trying to catch and systematize. Often modal words are used ontologically, to state that some beings or some facts are considered necessary, other facts being contingent. Seemingly, not everything that happens happens with the same degree of urgency; 3 divides 21 by

* To be presented in a joint symposium of the American Philosophical Association and the Association for Symbolic Logic on "Modal Logic" at the fifty-eighth annual meeting of the American Philosophical Association, Eastern Division, December 27, 1961.

necessity, but I read this paper by a caprice. In a somehow different way modalities are attributed not to facts themselves but to sentences referring to facts. True sentences are divided into necessary sentences and merely true sentences, false sentences into impossible and contingently false ones. All these habits may be called absolutist attitudes toward modalities. They have in common that a single being or fact or sentence is judged to be by itself necessary, contingent, or impossible. Absolutisms in modalities seem to be more or less remote relics of fantastic metaphysical doctrines.

The absolutist approach to modalities may be contrasted with the relative approach. According to the relativist attitude no sentence is necessary by itself, only relatively to other sentences. In the relative use, modalities refer to some particular relations between sets of sentences. It is difficult to attribute a reasonable interpretation to absolute modalities. But relative modalities seem fruitful methodological concepts, though in assigning precise interpretations to these words several nuances will have to be distinguished carefully. Some sentences are necessary with respect to other sentences; some sentences are possible with some other sentences. Two sentences may be each possible with respect to a system but impossible with each other. Thus, in the relative sense, modalities are closely affiliated with such concepts as compatibility, sufficiency, consequence, deducibility, entailment, etc. Sometimes one uses the assertory mode of speaking justifiably, and one asserts a sentence by itself as true. In more sophisticated circumstances, instead of asserting a simple sentence, one states a mode in which the sentence is related to a system of sentences. In ordinary discourse and in scientific comments the occurrence of modal words indicates that the topic is not known in its entirety. And this holds not only for possibilities but also for words seemingly referring to necessity. When you know that John went to Florence you do not say "John must have gone to Florence." Such a locution would indicate indirect inference rather than direct knowledge.—The notion of truth is not relative to a system. It is relative to a language as a whole. The notions of possibility, of necessity, and of other modalities, in contrast, may be relative not only to a language but also to a set of selected sentences of that language.

A relative use of necessity is well known from elementary logic textbooks, where it appears in describing the components of an implication or of a conditional. We say that the consequent is necessary (or is a necessary condition) for the antecedent of a true conditional and that the antecedent is sufficient (or is a sufficient condition) for the consequent. Thus, if "If John teaches Peter, than

language is efficient" is a true conditional, then the sentence "John teaches Peter" is a sufficient condition for the sentence "Language is efficient," and "Language is efficient" is a necessary condition for "John teaches Peter." "Is necessary for" or "is a necessary condition for" is a phrase that forms a grammatical sentence only when flanked by names. We cannot grammatically connect by it two sentences themselves in their normal forms. "Language is efficient is a necessary condition for John teaches Peter" (without quotes!) is not an English sentence at all. To express what we want we often form names of the component sentences. To say that the sentence "Language is efficient" is necessary for the sentence "John teaches Peter" is to translate into a metalanguage the conditional from the object language. However, one may also use the phrase "is necessary for" and remain in the object language. For that purpose one may use nominalized forms of the component sentences rather than names of those sentences. Thus "The efficiency of language is a necessary condition for John's teaching of Peter" is not in a metalanguage. It is in the object language, and it may be considered a paraphrase of the same conditional. For nominalized forms of sentences are not names of the sentences. Rather they are different forms of the very same sentences.

In affiliated though definitely different senses, the words "necessary" and "sufficient" are applied not to the components of a conditional but to the elements of an inference. One says that the set of premises forms a sufficient condition for the conclusion and that the conclusion is necessary for the set of premises. Here, however, one has to distinguish the concept of deducibility from the concept of consequence.[1] A sentence is deducible from a set of sentences if it is obtainable from this set by a finite number of applications of some specific rules of procedure. On the other hand, a sentence is a consequence[2] of a set of sentences if it remains true in any model in which every sentence of the starting set is true. The concept of consequence has semantical character, whereas the concept of deducibility is combinatorial. Modal concepts may be reinterpreted in either way, using either the deducibility relation or the semantic notion of a consequence. The results will not be the same. Let us, therefore, distinguish semantic necessity from combinatorial necessity. A sentence α is combinatorially necessary with respect to a set A of sentences if and only if α is deducible from A by means of some specific rules of inference. A sentence may or

[1] About this distinction see, e.g., Alonzo Church, *Introduction to Mathematical Logic*, vol. I, sec. 55 passim.

[2] This concept was introduced by Tarski. See, e.g., Alfred Tarski, *Logic, Semantics, Metamathematics*, pp. 409–420.

may not be combinatorially necessary for a set A, depending on what rules of inference are accepted. On the other hand, semantic necessity does not depend on any choice of rules. A sentence α is semantically necessary for a set A of sentences if and only if α is a consequence of A.

Often when people want to say that a sufficient premise is missing, they say that a premise is necessary: that is, for the conclusion in question such and such a premise is necessary; the explicitly stated premises are not sufficient. If α is not sufficient for γ but α and β together are sufficient for γ, then β is said to be—in this sense—necessary for γ. In other words "to be necessary for γ" in the sense of being needed is the same as "to be a factor in a conjunction for which γ is a semantically necessary condition, the remaining factors of the conjunction being not sufficient for γ." Of course, usually it is not true (and perhaps it is never true) that this or that particular premise is necessary in the sense (still another sense of "necessary") of not being replaceable by any other premise. It is one of the tasks of science and of intellectual work in general to find nontrivial sufficient conditions for known consequences, and the history of science provides plenty of examples when a condition thought to be necessary was replaced by a new condition inconsistent with the previous one and still sufficient for the consequences at hand. It is a banal mistake when from the acceptance of an enthymeme one concludes the acceptance of the most banal missing condition. To take the Jevonian example: All bodies moving in elliptic orbits are subject to the law of gravitation; therefore comets are subject to the law of gravitation. Here it is a naiveté to think of the sentence "Comets move in elliptic orbits" as the missing condition. Equally well, and perhaps better, one might take as an additional condition the following: "All bodies are subject to the law of gravitation, and comets are bodies."

Let us now turn to the concepts of possibility and their relations to necessity. Roughly speaking, a sentence is possible for a system if it can be added to the system without producing an inconsistency. Thus possibility is an extension of a system with the preservation of consistency. By consistency I mean here the so-called Post consistency: A system is consistent if there are sentences, or well-formed formulas, that are formulated in the language of the system but are not consequences of the system. Otherwise a system is inconsistent; i.e., it coincides with the totality of sentences, or formable formulas. It is customary to distinguish this notion of inconsistency from contradiction: A system is contradictory if it contains among its consequences two contradictory sentences, i.e., of the form α and $\sim\alpha$.

Let α, β be well-formed formulas in the language of a system S.

α is possible for S if and only if a system resulting from S by adjoining α is consistent. That is:

α is possible for S if and only if $\Sigma\beta \sim(\beta$ is a consequence of $\{S,\alpha\})$

(Capital "Σ" and "Π" are used for small and general quantifiers; "$\sim$" stands for negation, "$\{S,\alpha\}$" for the set composed of all elements of S and of α.)

The study of possibility is the study of consistent extensions of systems. For a system without consistent extensions there is no possible sentence that is not at the same time necessary. Such complete systems, as we know, are rare exceptions. More often we operate with systems admitting extensions and thus in connection with which it is appropriate to speak about possibility in a stricter or nonvacuous sense, according to which

α is possible, in the stricter sense, for S if and only if α is not a consequence of S and, when added to S, does not produce an inconsistency.

This is one of the ideas about possibility that can be found in Aristotle. For him a sentence is possible if it is not necessary and if, being assumed, it results in nothing impossible.[3] Possibility is a fruitful concept just because we are not omniscient. Had all our systems been complete, possibility would coincide with necessity and with theoremhood.

Examination of the logic of possibility, i.e., of consistent extensions of systems, may prove to be a delicate task. I shall illustrate the difficulties by only a few points.

It is maintained traditionally that (1) α is necessary if and only if $\sim\alpha$ is not possible, and that (2) α is possible if and only if $\sim\alpha$ is not necessary. However, these assertions can be seriously doubted, or at least they impose some strong conditions on the systems acceptable for study. Let us consider some of the difficulties involved in the first of these principles, supposedly fundamental to any system of modal logic.[4] As these assertions contain negative formulas, we should examine them only as applying to systems in which negation occurs, though the concepts of possibility and of necessity may be used for systems without negation as well.

The statement

(1) α is necessary for S if and only if $\sim(\sim\alpha$ is possible for $S)$

becomes here

α is a consequence of S if and only if $\sim\Sigma\beta \sim(\beta$ is a consequence of $\{S,\sim\alpha\})$

[3] For example, *Analytica Priora* 32a18–20.

[4] See, e.g., J. Łukasiewicz, *Aristotle's Syllogistic from the Standpoint of Modern Formal Logic*, 2nd ed., p. 135.

and

α is a consequence of S if and only if $\Pi\beta$ (β is a consequence of $\{S, \sim\alpha\}$)

This is composed of two statements:

(1a) If $\Pi\beta$ (β is a consequence of $\{S, \sim\alpha\}$), then α is a consequence of S

(1b) If α is a consequence of S, then $\Pi\beta$ (β is a consequence of $\{S, \sim\alpha\}$)

For the sake of transparency let us consider instead of (1a)

(1a') If $\Pi\beta$ (β is a consequence of $\{S, \alpha\}$), then $\sim\alpha$ is a consequence of S

which is obtained by putting $\sim\alpha$ for α and canceling the double negation. Incidentally, (1a') is the same as a component of (2).

In order that (1a') be true, we have to exclude systems with free variables. For instance, $\sim p$ is not a consequence of the sentential calculus, but p added to the sentential calculus makes every formula provable. Similarly, in any usual system with identity, $\sim x = y$ is not a theorem, being in flat contradiction with an axiom, but $x = y$ identifies all the elements and permits any identity as a consequence.

If one considers exclusively systems without free variables, further difficulties appear. A system of sentential calculus may not contain free variables if the usual axioms are preceded by quantifiers binding the variables and if the rules of inference state that if the closure of $\alpha \supset \beta$ and the closure of α are theorems, one can accept as a new theorem the closure of β (and a similar change in the rule of substitution, without allowing, however, substitution of phrases themselves containing quantifiers). Here, addition of Πpp makes the system inconsistent, but $\sim \Pi p\, p$ is not a consequence (for no formula with a quantifier under a functor is a consequence). This is not necessarily connected with the restriction on the rule of substitution. Let us return to the general theory of identity; it is based on the axioms:

$$\Pi x\ (x = x)$$
$$\Pi x\ \Pi y\ (x = y \supset y = x)$$
$$\Pi x\ \Pi y\ \Pi z\ ((x = y\ \&\ y = z) \supset x = z)$$

They do not give

$$\Sigma x\ (x = x)$$

as a consequence, for this is not true in an empty model in which the axioms are true. However, its negation

$$\Pi x \sim (x = x)$$

gives as a consequence any sentence whatsoever. For we have the closure of $p \supset (\sim p \supset q)$. Taking $x = x$ as p and detaching twice, we obtain that the closure of any formula is a consequence of the extended system. In order to maintain (1a') we have to restrict

ourselves in the understanding of the concept of necessity to non-empty models. Thus we make general impositions on our logical theories to rescue (1*a*′).

Let us now examine (1*b*). As it stands, it is false. Here is a counter-example. To the system that has as its axioms:

(Ax1) $\sim(p \supset q) \supset p$

(Ax2) $\sim(p \supset q) \supset \sim q$

and as its rules of inference:

(R1) The rule of substitution

(R2) $\alpha \supset \beta, \beta \supset \gamma \rightarrow \alpha \supset \gamma$

(R3) $\alpha \supset (\beta \supset \gamma), \alpha \supset \beta \rightarrow \alpha \supset \gamma$

(R4) $\sim\alpha \supset \beta, \sim\alpha \supset \sim\beta \rightarrow \alpha$

one can add $\sim\alpha$ (for any α whatever) as a new axiom without making the system inconsistent (though, of course, making the system contradictory). (Ax1), (Ax2), every derived theorem, and $\sim\alpha$ are all true in the following model:

$\supset$	1	2	3	$\sim$
*1	1	3	3	2
*2	1	1	1	1
3	1	1	1	1

where 1 and 2 are designated (true) values. Not every formula, however, is true in this model. Taking such a formula as α, we obtain a counterexample for (1*b*). This would not be surprising if it were not for the fact that this system contains all and only those formulas as theorems which are normal two-value tautologies and, thus, can be considered to be a formalization of logically true sentences.[5] The rule of detachment and several other valid rules of inference are not derivable rules for this system. In order to save (1*b*) one has to exclude such systems of logic, namely, systems that, though complete in the usual sense, are metalogically incomplete.

We shall meet further difficulties if we consider definitions. Definitions are classical examples of possible sentences. It is possible to accept as a definition

$$\Pi p\, \Pi q\, (p \vee q \equiv \sim p \supset q)$$

if our system already contains negation, implication, equivalence, but does not contain the sign of alternation in any other role. It is possible to accept this definition because it does not lead to the

[5] See, e.g., *The Journal of Symbolic Logic*, **24**: 193–202. Other systems with somewhat similar properties were presented by Alan Ross Anderson and Nuel D. Belnap, Jr. (*ibid.*, **24**: 301–302) and by Jean Porte (*Comptes rendus des séances de l'académie des sciences*, **251**: 188–189).

acceptance of all sentences provided the system so far has been consistent. However, it would not be possible to accept

$$\Pi p\, \Pi q\, (p \vee p \equiv \sim p \supset q)$$

for it leads to inconsistency. Namely, taking $q/\sim p$,

$$\Pi p\, (p \vee p \equiv \sim p \supset \sim p)$$

thus

$$\Pi p\, (p \vee p)$$

and

$$\Pi p\, \Pi q\, (\sim p \supset q)$$

Taking

$$p/\sim(p \supset p)$$

and detaching, we have

$$\Pi q\, q$$

This definition is, therefore, impossible.

Admission of a possible definition limits the possibility of other definitions. Thus, admitting the definition of alternation, we form a new system for which the following is not a possible definition any more:

$$\Pi p\, \Pi q\, (p \vee q \equiv \sim(p \supset \sim q))$$

though this definition was possible before. The two definitions are, only, not co-possible.

Generally, extending a system by a possible sentence limits the possibilities and extends the necessities.

A fundamental problem of modal logic is: What are the possibilities for a system that admits a possibility in the stricter sense? That is, if there is a sentence that can be added to S without inconsistency, are there other possible sentences for the system S? And how are they interconnected? A crucial result in this line is due to Lindenbaum: For a consistent system S there is always a set S' of sentences possible and co-possible (possible together) for S such that no sentence outside this set is possible for $\{S,S'\}$. In other words, if there is a possible sentence, there is also a maximal set of co-possible sentences. In some cases this maximal set is known to be unique (e.g., for some fragments of the sentential calculus[6]).

Under some general conditions if there is a sentence in the stricter sense possible for S, then there are infinitely many non-equivalent sentences each of which is possible in the stricter sense for S. (Two sentences are here called equivalent if they are con-

[6] Tarski, *op. cit.*, pp. 293–400.

sequences of each other with respect to S; i.e., if any time one is true in a model in which all the sentences of S are true, the other is also true in this model.) Thus, in many cases, possibilities do not appear alone, but rather in infinite choices.

If by modal logic one understands the study of extensions of systems, their variety and the variety of their interconnections, modal logic will become an important part of the study of the foundations of our knowledge. And several ideas from previous works on modalities will find their precise reformulation.

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LUCK, LICENSE, & LINGO*

FEELINGS of doubt and queasiness are common enough in philosophy. They are practically prerequisites for its pursuit. And when you manage to suppress them in one place, with regard to one problem, they are extraordinarily likely to turn up again not far away. So to do philosophy you must at times be something of a diagnostician, exploring the philosophical geography of your own anxieties, with the idea being not so much to remove them as to have them located, parceled, and appropriated for constructive purposes.

Now the present exploration takes Goodman's ***Fact, Fiction & Forecast***[1] as its home base, with the problem at hand that of providing justification for inductive inference. This is a problem which, in one form or another, has long been with us, and I shall argue that if, after digesting Goodman's contributions and coming face to face with his "new riddle of induction," one *still* has the gnawing feeling that there is more to be said, then one's anxieties will have shifted to a new location.

Goodman's initial joust with the problem:

> How do we justify a *deduction?* Plainly, by showing that it conforms to the general rules of deductive inference. . . . Rules and particular inferences alike are justified by being brought into agreement with each other. *A rule is amended if it yields an inference we are unwilling to accept; an inference is rejected if it violates a rule we are unwilling to amend* . . . and in the

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[1] Nelson Goodman, *Fact, Fiction & Forecast* (Cambridge, Mass.: Harvard, 1955).

agreement achieved lies the only justification needed for either. All this applies equally well to induction. . . . Predictions are justified if they conform to valid canons of induction; and the canons are valid if they accurately codify accepted inductive practice.[2]

So what is called for in the justification of deductive or inductive inference is codification, codification of the kind of inference in question. But these appear to be theses of different orders. If we draw the traditional distinction, we might say that deductive inference is essentially organizational, tells us "nothing new," whereas induction purports to generate fresh knowledge. Now it is one thing to hold that individual attempts at organization stand or fall with our general intuitions about how knowledge may be organized (made explicit in deductive rules). We may, after all, organize our data however we wish, and all that is demanded is that our statement of organizational principles accurately reflect our practice, that they mesh together. But it is something else again to maintain that we may go beyond the evidence provided that we do so in accordance with summary statements of how we find ourselves inclined to go beyond the evidence. This appeal to codification has a more unpleasantly circular feel than the other. We seem, as it were, to be owed less freedom in deciding what predictions to make than in choosing which of our facts to redigest.

In so far as this comes to an argument for Goodman's view on deduction (which I shall support elsewhere and cease to worry about here) and against his view on induction, I shall confide that I am unconvinced by it, partly, but only partly, because I am not entirely happy with the "traditional distinction" involved. But accepting the distinction as it stands for a moment, it might be pointed out that we should have expected to have more lingering qualms in the inductive case, if only because we presumably had more qualms there to begin with. Deductive inference never really seemed in need of support or justification in the first place, though saying exactly why is far from easy.

There have been numerous attempts to justify inductive inference. D. C. Williams's argument from the statistical syllogism [3] has long fascinated me. A population is as likely to resemble a good-sized sample of it as a good-sized sample is to resemble the population. And elementary statistics provides us with reasons for saying that this is quite likely indeed, though "likely" in a sense which Williams never quite gets all the thorns out of. Then

[2] *Ibid.*, pp. 66–67.

[3] Donald Williams, *The Ground of Induction* (Cambridge, Mass.: Harvard, 1957).

there is the Peirce notion of induction as self-correcting, more recently developed and defended by von Wright, who concludes that "a reasoned policy for purposes of prediction and generalization is necessarily equivalent to an *inductive* policy. If we wish to call reasoned policies *better* than not-reasoned ones, it follows further that induction is of necessity the *best* way of foretelling the future."[4] So Williams claims to show that the future, or the unexamined cases, are all but bound to resemble the past, or the examined ones. And von Wright, construed a bit liberally, argues that no policy can be more reasonable than one which anticipates such resemblances. But if we look the new riddle of induction in the eye, it must follow that neither the arguments cited nor any other of their kind can be of any help to us, for the crucial question always remains: *resemble in which respects?*

For Goodman shows that if we are sufficiently ingenious in defining new predicates, any amount of evidence for one empirical hypothesis can stand equally well as evidence for a hypothesis conflicting with it. Give me reasons for your prediction and I can twist them, by impeccable logic, into reasons for a contrary prediction. You say the next emerald will be green, I say it will be grue,[5] and we use the same evidence and the same predictive principles. Now to arrive at this unwelcome result we must be prepared to employ unusual predicates. Those needed for the generation of conflicts will in general be foreign to our language—and to any other, until we invent them. But Goodman argues that there can be no a priori grounds (to put it baldly, and in terms at which he might demur) for distinguishing predicates that are troublesome from those which are trouble-free, for marking off the artificial or *positional* predicates from the genuine or *qualitative*. Now I want to note that even if one could explicate perfectly the idea of a genuine or purely qualitative or *projectible* (from Goodman) predicate, it would be of no help whatsoever for purposes of resurrecting a priori justifications of inductive inferences or policies. For there is no law of logic (deductive *or* inductive) according to which a distinction may be employed arbitrarily in an argument just because it has been drawn with clarity and precision. If one were troubled initially by Hume's problem as it has come to be understood, I cannot see why one should be much less troubled to learn that the legitimate inductive inferences can be segregated from the illegitimate by application of the notion of qualitative predicate. And that version of Hume's

[4] Georg von Wright, *The Logical Problem of Induction* (2nd ed., Oxford: Basil Blackwell), p. 174.

[5] Goodman, *op. cit.*, p. 74 ff.

problem is certainly the object of Williams's attack and presumably the object of the Pierce–von Wright militations. I take the upshot to be that a-priorist thrusts at the inductive dragon cannot provide justification for any inductive inference with actual content, can justify no more than the form that inductive arguments take, which, as I see it, is to justify nothing at all.

But Goodman, as we know, is not concerned with the old form of Hume's problem. For him "the problem of induction is not a problem of demonstration but a problem of defining the difference between valid and invalid predictions."[6] To this end he outlines a theory of projection, whose largest task it is to weed out the spurious hypotheses with cooked-up predicates that gave rise to the new riddle. Here the right, or projectible, predicates turn out to be essentially the well-*entrenched* ones, those with long histories of use. (The theory is rather more elaborate than this sketchy reminder of its content.) Thus the respects in which we may validly predict resemblance of future cases to past ones are mainly those identifiable by predicates with decent backgrounds.

This allows an explication of 'valid prediction' which uses none of the many philosophical modes toward which Goodman has expressed discomfort. 'Entrenched' is a manifest predicate, not a dispositional one. And Goodman anticipates the reaction likely from those whose concern has been with older forms of the problem: "The reason why only the right predicates happen so luckily to have become well entrenched is just that the well entrenched predicates have thereby become the right ones."[7]

This is where I want to stop and reflect. The bona fide predicates are those which are much with us, and they are bona fide *because* they are much with us. Of course, had we accepted, with Goodman, the dictum that explication was all we wanted, we should not have found ourselves talking in this way, and I suspect that Goodman says what he says in this connection only to throw a bone to those who are not wholly convinced—or, perhaps, to tease them a bit. Nonetheless, the feeling of relief may not yet be complete. What can we say?

Surely one of the most exciting intellectual spectacles available to us is the history of physical science. You can be intrigued and fascinated in any number of ways when you read of the growth and development of physical theory. Now what I find overwhelmingly remarkable in scientific history is the extent to which what might be called extrapolations, many of them mammoth, have

[6] *Ibid.*, p. 68.
[7] *Ibid.*, p. 98.

been successful. The contributions of Newton, Huygens, Maxwell, to name only three, were in large measure made possible by their willingness to apply ideas known to be useful in one domain in other areas. Use of analogy for extension of one's knowledge always has, in essence, this character. You have concepts, equations, or theories which help to organize for you one class of phenomena, and although there is no logical guarantee whatsoever that they should be of service outside that class, again and again they turn out to be so. Of course it isn't that simple. There is always a problem of *how* to extrapolate, and for every instance of what I call successful extrapolation in the history of science there must have been thousands that failed to bear fruit. And there are cases where extrapolations almost beg to be made, then turn out not to work. The failure of Newtonian physics to apply to the microscopic realm strikes me as a case of exactly this kind. On the one hand, why should it have applied here, in a realm beyond that of its evidence? Yet it was most surprising that it failed to. We have come to have a very great confidence that concepts which help us in one area of our experience will turn out to be extendible to other areas, and on a very large scale. Indeed, having learned that the notion of indeterminacy is useful in explaining the phenomena of quanta, those who take seriously the old free-will issue have generated immense hopes (and rather uncontrolled arguments) to the effect that such a notion must apply in the field of human action. I certainly don't want to talk about that here, but I want to urge that whatever plausibility extrapolative arguments, on either grand or minute scale, may have is reinforced tremendously by our backlog of successes with such arguments. Now clearly I am not offering a precise description of this extrapolative phenomenon, much less an explanation of it. But I am saying that one can come to feel that there is a very remarkable phenomenon in evidence here, one which couldn't have been counted on, to put the matter in what is perhaps a revoltingly cosmic way.

And I feel much the same about our simpler concepts, even those as simple as concepts of colors or hues. Here it is not so much that there is out-and-out extrapolation from one domain to another. It is that the notions of these basic properties that we learn to identify (a clause which masks a multitude of problems) are so uniformly of use in describing and organizing our experience. Lest there be misunderstanding, I remark that I am saying the strongest thing that I could be taken to be saying. I am saying that I find it remarkable that we get any mileage at all out

of our concepts, that we have any use whatsoever for general terms (to import an expression that I prefer to 'predicates').

How is it that general terms turn out to be of use for us? Now it is a brute and happy fact that human beings are able to learn, ultimately by ostension, how to use such terms. We, and with us other creatures, are able to achieve *recognition* of what the myth makers have reified as properties. (Admittedly it may be a happy myth; some of our most noteworthy philosophy is happy myth.) But you can explain what this ability consists in only up to a point. Any explanation of how we learn to apply a specified general term will itself make use of general terms, which—far from being a defect—is reflective of a basic feature of language and a *sine qua non* of it (the need for general terms or their equivalents in saying *anything*). You can point out that we see a few examples of blue things and we catch on, if judiciously taught, how and when to apply 'blue', but what more can you say? You certainly explain nothing if you say that we learn to apply 'blue' to blue things. And you only push the problem further back if you say that we learn to apply 'blue' to things sufficiently similar in color to the initial specimens. For in what does similarity in color consist? Somewhere you must terminate the attempt at explanation and admit that it simply *is* possible to learn the use of general terms, and possible in such a way that individuals can achieve agreement in their use. I think there is a natural temptation to give too much credit to nature for all this, as if objects and simple properties appeared for us already labeled, and our only job was to read the labels (or as if they carried membership cards for exclusive clubs known as *natural kinds*). Concepts and the employment of general terms depend upon classifications, and, as I like to put it, one class is as good as another as far as logic is concerned. Yet from the vast number of theoretically available alternatives we mark off a relatively small number of (open-ended) classes, which are roughly the same for me as for you, and in terms of them we find it possible to make sense out of our experience, out of the "buzzing, booming confusion." I am not about to heap on us the credit that I have begrudged nature. For it is clear that we find it quite natural and immediate to take certain relationships as instances of similarity and identity, and these already begin to mark out the classes that we choose—classes which, looked at the other way round, are themselves definitive of what is to count as a similarity or an identity. I suppose this is all very *extensional*. But such an orientation is necessary if we are to dig under, for the moment, the topsoil in which our con-

ceptual and linguistic shrubbery grows. One who sees no need for such digging is presumably satisfied with an *intensional* account of the topsoil's richness. To be so satisfied is to have one less problem to face, but it is also to be denied here the occasion for an attitude of wonderment.

If one feels an explanation coming on it will likely be an evolutionary one. You can say that our ability to mark off classes and gain concepts that can serve us has developed much as our species has developed. Or you can contrapose and say that were it not for the evolvement of such a knack we should have had no language, no conceptual power, and very little chance for survival through these many centuries. Lewis gives us something of this kind when he says:

> Our concepts are devised with purpose to catch the significant, the subject of meaningful generalization, at whatever level and in whatever way we may. When particular concepts fail, we merely abandon them . . . in favor of corrected ones, which take cognizance of, and include the ground of, our previous failure. That conception *in general* should be invalid, is quite impossible. The attempt to envisage an experience or state of affairs such that *every* attempt to discover stabilities must fail, is the attempt to conceive the inconceivable.[8]

(Note that *discovery* of stabilities already presupposes conception.) Of course Lewis's sense of 'impossible' is the pragmatic one: *impossible,* given that we are here and talking about it. And if were weren't, there would be no problem for us. This probably cuts as deep as it is safe to go, as deep as there is use for going. But that needn't mean that it is fully satisfying.

We do quite well at predicting, and enjoy physical theories of enormous scope. These happy facts I find remarkable. Conceptualization and language I find remarkable as well. And, in a sense imitative of the mathematician's, I find the first feeling reducible to the second. That we have predicates that are successfully projectible is, so to speak, a special case of our having any use for predicates at all. What bothers me most about conventional treatments of inductive inference is that they are so much about inference. A class-and-property logic has been handed down to us, and all too uncritically we find ourselves employing it and the schematization to which it lends itself, all too insensitive to the thought that the problems we generate are in large measure problems regarding the applicability of the logic, the schematization, itself. Of course you can't question everything at once, and in

[8] Clarence Irving Lewis, *Mind and the World-Order* (New York: Scribner's, 1929), p. 385.

philosophy you keep on finding that just the things you want most to question can't sensibly be questioned at all. Nonetheless, it seems to me that whatever restlessness we might still feel when taking Hume's problem, old style, seriously, might best be treated by turning our attention toward the origin and use of language in its most basic dimensions. We might thus achieve at least a parceling of our perplexities.

I have one other misgiving about conventional treatments of induction, and it is not new. It is that the examples to which our attention is directed are so frequently of such simple form. We are encouraged to believe that, once we have dealt with the simple cases, those which are more complex will fall into place, that they proceed by the same logic. Now what is to count as the *same logic* is up to us to decide; we may mark out a class of patterns of inference however we choose. But treatment of the highly sophisticated, multiply nested inferences of mature science *or mature common sense* may call for rather different techniques from treatment of simpler cases. I note only that appropriate attention to the actual functioning of language, both plain and specialized, is called for in this too.

Back to Goodman's "the well entrenched predicates have thereby become the right ones." Could an essentially different set (not of predicates, but of extensions or concepts) have served as well—have served better? But this is like asking how well off we might have been with a basically different conceptual scheme, if you will allow me that phrase, or a radically different language. And almost any answer except a shrug of the shoulders seems out of place, though one might be safe enough in admitting that logic seems not to demand precisely our conceptual structure for the organization of what we call our experience. Perhaps it would not be too far from Goodman's meaning to read his dictum as "We use the predicates (and concepts) we do because they are the ones we have."

Now to pick a quarrel in my last paragraph. Williams chides, "If there is any puzzle about induction, I fear, it is how we manage to go wrong as often as we do."[9] I think we deserve more credit, that the entire fabric of our language is requisite for our doing as well as we do. But as for the possibility of such a language, as for the question "Why not nothing?" I can only answer: because we are very much in luck.

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[9] Williams, *op. cit.*, p. 167.

EVIDENCE AS JUSTIFICATION *

Morality requires, not only that a man should act according to his judgment, but that he should use the best means in his power that his judgment be according to truth. If he fail in either of these points, he is worthy of blame; but, if he fail in neither, I see not wherein he can be blamed.—Thomas Reid.[1]

1. I shall discuss certain philosophical questions suggested by the concept of *evidence*—the concept of a statement (proposition, hypothesis, belief, or judgment) being one that is *evident* or one that constitutes our *evidence* for some other statement. The terms "evident" and "evidence" will be taken in the way in which philosophers ordinarily take them in such statements as: "If a man knows, concerning some statement, that that statement is true, then he has adequate evidence for that statement, or all the evidence he needs for asserting the statement"; "It is evident that there can be no life, as we know it, on the moon"; and "In the application of inductive logic, the total evidence available must be taken as basis for determining the degree of confirmation." The paper is concerned, then, with the *epistemological* concept of justification and with some of the relations holding between this and the *ethical* concept of justification.

I shall consider, first, the possibility that *evidence* is an ethical concept definable in terms of *right* or *duty;* I shall consider several possible definitions of "evident" and note the theoretical advantages of two of these. Then I shall consider the question whether it is possible to formulate *rules* of evidence—rules telling us the conditions under which a statement may be said to be evident or to have evidence conferred upon it.

2. Perhaps it is true that a statement is *evident* for a certain person provided only that the person is *justified* in accepting the statement. But what sense of "justify" is this? Could we say, "A statement e is evident to a person S provided only S has the right to accept e and the duty not to accept not-e," where "right" and "duty" mean what they ordinarily would mean in such statements as "No one has the right to take another man's life" and "It is our duty to be charitable to all living things"?

There are many objections that can be made to the view that evidence is a kind of right, or duty, to believe. The most convincing of these, I think, is the following: A man may have the (ethical or moral) right to accept a statement which is not evident; indeed

* To be presented in a symposium on "Justification" at the fifty-eighth annual meeting of the American Philosophical Association, Eastern Division, December 29, 1961.

[1] *Essays on the Active Powers of Man,* essay V, ch. IV. Hamilton's edition of Reid's *Works,* p. 647.

he may have the duty to accept a statement which is not evident. For example, a man may have the right to believe that the members of his family are honest or faithful even though he does not have adequate evidence for such beliefs. A sick man may have the right to accept certain theological statements for which he has no evidence; one might argue that, if by accepting such statements the man could make himself well and useful again, then he has the duty as well as the right to accept them.[2]

To say, then, that a man is epistemically justified in accepting a certain statement is not to say that he is ethically or morally justified in accepting that statement. But it may yet be possible, by some more complex device, to construe epistemic justification as a kind of ethical or moral justification. I shall consider two possibilities.

3. The first of the two possibilities is suggested by what Carnap has called "the principle of total evidence": viz., "In the application of inductive logic to a given knowledge situation, the total evidence available must be taken as basis for determining the degree of confirmation."[3] Let us note that the principle is a "requirement"; it tells us that we *must* use the total evidence that we have. Now we may ask, as Ayer has asked: "But why *have* we to take as evidence the total evidence available to us. . . ? What sort of principle is this?"[4]

Oversimplifying, for the moment, we may say: the requirement tells us that at any time at which a man makes a decision he must base his decision on the total evidence he has at that time. We can make the requirement more plausible as a requirement if we add a second "must": at any time at which a man *must* make a decision, he must base his decision on the total evidence that he has. Ayer now asks us: Why *must* he base his decision on the total evidence available? Suppose we said this:

To say that a statement e is *evident*, to a person S at a certain time t, is to say that, if S must make a decision at t, then S must base his decision upon e.

[2] These points are clearly made by Roderick Firth in "Ultimate Evidence," this JOURNAL, **53** (1956): 732–739, and in "Chisholm and the Ethics of Belief," *Philosophical Review*, **68** (1959): 493–506. Compare also H. H. Price, "Belief and Will," *Aristotelian Society Supplementary Volume XXVIII* (1954), pp. 1–26.

[3] Rudolf Carnap, *Logical Foundations of Probability* (Chicago, 1950), p. 211.

[4] A. J. Ayer, "The Conception of Probability as a Logical Relation," in *Observation and Interpretation* (London, 1957), ed. Stephan Körner; reprinted in *The Structure of Scientific Thought* (Boston, 1960), ed., Edward Madden. Compare C. G. Hempel, "Inductive Inconsistencies," *Synthèse*, **12** (1960): 439–469; esp. 450–455.

If we ignored the obvious oversimplification that this definition of "evident" involves, we could say that the definition has the advantage of turning the total-evidence requirement into a logical truth and thus removing the puzzlement the requirement otherwise seems to engender. For the requirement (in its oversimplified form) would now become: at any time at which a man must make a decision, he must base his decision on all those statements which at that time are such that, if he must then make a decision, he must base his decision on those statements.

An adequate definition of "evident" along such lines, however, would require a more exact formulation of the total-evidence requirement. A possible formulation is the following, but I am not at all sure that it is adequate.

For any statement h and any set of statements E, there can be derived, by applying the principles of inductive logic or probability, a set of probability-statements P of the following sort: the members of P state the probability, in relation to E, of h and of statements comparing the utility of making a stake upon h should h be true with the disutility of making a stake upon h should h be false. And from any such set of probability-statements P there can be derived, by applying the principles of ethics and of rational decision theory to P, a set of ought-statements O, prescribing how much ought to be staked on h and how much ought to be staked on not-h. The requirement of total evidence might now be put in this way: whenever a man is obligated, at a certain time t, to decide how much to stake at t upon a given statement h or how much to stake upon not-h, then from the set of all and only those statements which are evident to him at t, he should derive, in the manner just described, a set of ought-statements O, prescribing how much he ought to stake upon h and how much upon not-h. (And then, of course, he should set out to make the stakes prescribed.) Our definition of "evident" would now become the following. To say that a statement is *evident* to someone S at a certain time t is to say this: if S is obligated at t to decide how much to stake upon some statement h or upon not-h, then e is a member of that set of statements from which S ought to derive, in the manner just described, a set of ought-statements prescribing how much he ought to stake on h and how much on not-h.

Such a definition, even if it could be formulated precisely, would presuppose that inductive logic, ethics, and rational decision theory have been sufficiently developed so that we can derive, for any statement, a set of ought-statements of the sort I have tried to describe. It would also presuppose that the requirement of total evidence is a proper requirement—a presupposition which

might be challenged on the ground that, in practice, few of us are able to conform to the requirement.

Is there any other way of formulating an ethical definition of "evident"?

4. Keynes wrote, in *A Treatise on Probability:* "We might put it . . . that the probable is the hypothesis on which it is rational for us to act. It is, however, not so simple as this, for the obvious reason that of two hypotheses it may be rational to act on the less probable if it leads to the greater good."[5] Consider two hypotheses thought to be such that acting on the one would lead to the *same* good as would acting on the other: could we say that of two such hypotheses the *more probable* is the one on which we ought to act—and could we then go on and define "evident" by means of a similar device? Here, too, there are difficulties in formulating precisely the conditions that are required, and the best I can do is to suggest a program for a definition.

Keynes said that we should believe the probable "in *preference* to" the improbable; this, he added, is "a loose way of expressing the propriety of *acting* on one hypothesis rather than on another."[6] Suppose we try to define the locution "*S* must prefer *h* to *i*" in the following way: "For any good *G*, if *S* were permitted to choose between (i) risking just this good *G* against a greater good on *h* and (ii) making the same risk on *i*, he would be obligated to risk it on *h*." ("*S may* prefer *h* to *i*" would be defined by a similar conditional, except that the consequent would say, "he may risk it on *h*"; analogously for "need not prefer" and "must not prefer.") The terms of our defining conditional are, of course, far from clear.[7] But, I should think, they are terms we must use in any account of rational action; I suggest, therefore, that if we can define "probable" and then "evidence" in some such way as this, we shall have made some progress.

We cannot define "*h* is evident to *S*" as meaning that *S* must prefer *h* to not-*h*. For, as Keynes suggested, if we can say of a statement *h* that *h* is merely probable and not evident (i.e., if *h* is not evident but is more probable than not in relation to what *is* evident), then *h* must be preferred to not-*h*. Thus Bishop Butler had said, in the Preface to the *Analogy*, that, where we have only a

[5] J. M. Keynes, *Treatise on Probability* (London, 1921), p. 307.

[6] *Loc. cit.*

[7] I have tried to throw light upon some of them in "What Is It to Act upon a Proposition?" forthcoming in *Analysis*. "Risk" must be so interpreted above that (*a*) it refers to over-all risk and thus to possible winnings and losings, and (*b*) a man may be said to risk whatever it is he believes he risks.

low probability and "more satisfactory evidence cannot be had," we are, in matters of practice, "under an absolute and formal obligation" to act upon that low probability.

Suppose, however, we were to define "probable" in the way just described. Let us consider saying, of the preanalytic, epistemic concept of *probable* (the concept of *being more probable than not*) that it is definable in ethical terms as follows: "*h* is probable for *S*" means that *S* must prefer *h* to not-*h*; and "*h* is *more probable* than *i* for *S*" would mean, similarly, that *S* must prefer *h* to *i*.[8]

We could now define "evident" in this way: "*h* is *evident* to *S*" means that (*a*) S must prefer *h* to not-*h* and (*b*) there is no statement *i* such that *S* must prefer *i* to *h*. Any statement that is evident is, therefore, also probable.

If these definitions are adequate, then, I think, we have the means of defining most, if not all, of the other epistemic concepts we need.

5. For simplicity let us eliminate the reference to a person *S* and say "*h* must be preferred to *i*" instead of "*S* must prefer *h* to *i*." And let us remind ourselves that "need not" is the contradictory of "must" and that "may" is the contradictory of "must not"; i.e., that *S* must do *A* if and only if it is false that *S* need not do *A*, and *S* must not do *A* if and only if it is false that *S* may do *A*. (Note also the parenthetical remark about these expressions in the preceding section.)

We have said that a statement *h* is *probable* provided *h* must be preferred to not-*h*. We may now add that *h* is *acceptable* provided *h* may be preferred to not-*h*; thus all probable statements are acceptable, but not conversely. Let us say further that *h* is *unacceptable* provided *h* must not be preferred to not-*h* and that *h* is *gratuitous* provided *h* need not be preferred to not-*h*; thus all unacceptable statements are gratuitous, but not conversely.

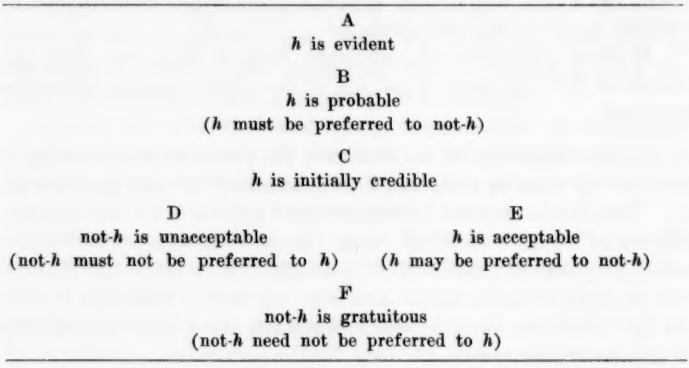
We may add further that a statement *h* is *initially credible* provided that *h* is acceptable and not-*h* is unacceptable; thus all initially credible statements are acceptable, but not conversely. To say of a statement that it is initially credible is to say, in effect, that there is some presumption, however slight, in its favor; while to say of a statement that it is acceptable is to say, in effect, that there is nothing to be said against it. (Taking "probable" and

[8] Using "probable" in this way, we could say that one of the tasks of inductive logic is that of describing the conditions under which statements that are evident make probable—i.e., confer probability upon—statements that are not evident.

"initially credible" in these epistemic senses, we cannot say of any initially credible statement that its denial is probable.)

A statement *h* may be said to be *initially incredible* provided that not-*h* is initially credible, just as *h* may be said to be *improbable* provided *h* is probable. And we may say that *h* is (epistemically) *indifferent* provided that *h* is acceptable and not-*h* is also acceptable. If we choose, we may add still another epistemic category: a statement *h* should be *withheld* provided *h* is unacceptable and not-h is also unacceptable; presumably only paradoxical statements would fall in this category.

The relations among some of these epistemic categories may be indicated by the accompanying diagram. Each line implies what



falls below it, and none implies what is above it (but D and E, which are beside each other and independent of each other, together imply C, which is above them).

It may easily be calculated that other possibilities, with reference to the diagram, are: not-B (*h* is gratuitous); not-D (not-*h* is acceptable); not-E (*h* is unacceptable); not-F (*h* is improbable = not-*h* is probable); D and not-E (*h* should be withheld = not-*h* should be withheld); not-D and E (*h* is indifferent = not-*h* is indifferent); and not-D and not-E (not-*h* is initially credible = *h* is initially incredible).

6. Can we formulate rules of evidence—rules enabling us to decide which, of various possible statements, are statements that are evident? I suggest the following as a procedure which may enable us to make explicit rules of evidence for empirical, or nonlogical, statements.

Let us consider a reasonable man for whom some (nonlogical) statement *e* is evident and consider the way in which he might

justify his counting *e* as a statement that is evident. How would he reply to a series of Socratic questions of the type, "What is your evidence for *e*?" or "What is your justification for counting *e* as evident?"? (It is essential not to confuse these questions with "How did you come to believe *e*, or find out that *e* is true?," "How would you go about persuading some other reasonable person that *e* is true?," or "What would further confirm that *e* is true?" It is also essential to note that our questions need not imply or presuppose that we are challenging or doubting the man's justification for counting *e* as a statement that is evident; we are seeking only to elicit the nature of this justification.)

For many statements *e*, for which our reasonable man has adequate evidence, he would answer our question, "What is your evidence for *e*?" or "What is your justification for counting *e* as evident?," in the following way: "My justification for counting *e* as evident is the fact that (*a*) *f* is evident and that (*b*) if *f* is evident then *e* is evident." We may then, of course, ask him for his justification for counting *f* as evident, if not also for his justification for the statement that if *f* is evident then *e* is evident.

Is there a proper stopping place for such questions? If a man offers the statement "The ground is covered with snow this morning" as his evidence for something else and if he justifies his statement by saying, "I *see* that there is snow on the ground this morning," we cannot, of course, ask him for his justification or his evidence for *seeing* that there is snow on the ground. But this does not mean that we have reached a stopping place in our cross-examination. For we can go on to ask him: "What is your evidence for the statement that it is *snow* you see there on the ground?" or "What is your justification for thinking it is *snow* that you see?" And to these questions, extraordinary as they may be, our reasonable man will have an answer: "It obviously looks like snow; this is the time of year for it; nothing else, right here under these conditions, would have produced a similar effect. . . ." Hence no statement of the form "I see that *a* if *F*" need put a stop to our questions. Analogous considerations hold for statements beginning, "I perceive . . . ," "I observe . . . ," "I know. . . ." Therefore perception, observation, and knowledge do not provide stopping places for our questioning.

We should remind ourselves, moreover, that perception words do not always take propositional objects and that in their non-propositional use they tell us little or nothing about what the perceiver is justified in believing or about what statements are evident for him. Before Eichmann was captured in Argentina,

there were, presumably, many people who saw him there, but very few of these people saw *that* Eichmann was there. In the true statement, "Many people saw Eichmann there," we may substitute for the word "Eichmann" any other description that applies to the same thing, e.g., "the beast of Auschwitz," and the result will be true; but the result will tell us nothing about what anyone was justified in believing. These considerations indicate, I think, the inadequacy of such simple "acceptance rules" as: "If a man sees an *F* then he is justified in accepting the statement 'There is an *F*.'"

What would be a stopping place, then? I shall propose what might be called the philosophically orthodox answer to this question.

Belief-statements would seem to provide one such stopping place. Let us suppose our man is led to say "I believe that Socrates is mortal," and we ask him "What is your evidence for the statement that you believe that Socrates is mortal?" or "What is your justification for counting as evident the statement that you believe that Socrates is mortal?" To these strange questions, I suggest, our man could reply in the following way: "My evidence for the statement that I believe that Socrates is mortal is the fact that I do believe that Socrates is mortal" or "My justification for counting as evident the statement that I believe that Socrates is mortal is the fact that I believe that Socrates is mortal." In this case, the man has justified a statement merely by reiterating it; the statement's evidence is what the statement says. Note that this type of reply is not appropriate to the questions with which we began. To the question "What justification do you have for counting as evident the statement that there can be no life on the moon?" it would be inappropriate—and impertinent—simply to reiterate, "There can be no life on the moon." But, I am suggesting, we can state our justification for certain statements about our *beliefs* merely by reiterating those statements.

It is tempting, at this point, to introduce the term "self-evidence." But there are extraneous issues that have come to be associated with this term; to avoid these issues I shall introduce certain other technical terms instead.

I shall use Meinong's term and say that beliefs are "self-presenting." Let us say, generally, that a man's state (e.g., his believing that Socrates is mortal) is *self-presenting* to him provided it fulfills the following conditions: it is evident to him that he is in that state; and his evidence for the statement that he is in that state, or his justification for counting as evident the statement

that he is in that state, is the fact that he *is* in that state. (There are those who would reject the statements in the definiens because of a disinclination to say, of a man, that it is *evident* to him that he believes that Socrates is mortal. For such philosophers, I think we can propose an alternative definition of "self-presenting" which will also serve the purposes of the original definition. We may say, for these philosophers, that a man's state is *self-presenting* provided: (*a*) it is not evident to him that he is in that state, or "it is inappropriate to say that it is evident to him" that he is in that state; and (*b*) the fact that he is in that state can serve as a part of his evidence for certain other beliefs. On this interpretation, we cannot say that my believing that Socrates is mortal is evident to me; nevertheless it is part of my evidence that Jones, who has the same belief, agrees with me about the mortality of Socrates.) Let us say, of a man who is in a self-presenting state, that the statement that he is in that state is one which, for him, is *directly* justified. As long as I believe that Socrates is mortal, then the statement "I believe that Socrates is mortal" is one which, for me, is directly justified.[9]

Believing may thus be self-presenting. So, too, for those states described by "thinking that one remembers that . . ." or "seeming to remember that . . ." (as distinguished from "remembering that . . ."), and "taking" or "thinking that one perceives," as distinguished from "perceiving." Other psychological attitudes—thinking, desiring, hoping, wondering, wishing, loving, hating—and also, of course, appearing (or, as I should prefer to put it, "being appeared to")—may also be self-presenting. There are various philosophical questions we can ask concerning such states. For example: If a certain state, such as believing that Socrates is mortal, is sometimes self-presenting to a man who is in that state, is it always self-presenting to him when he is in that state? If a certain state is self-presenting, is the state of not being in that state also self-presenting (to a man in normal psychological condition)? And if a state is self-presenting for one man, is it also self-presenting for another; that is, if my believing that Socrates

[9] Meinong used the term "self-presentation" in a way similar to the way in which I intend "self-presenting," but without defining it as above; see his *Über emotionale Präsentation* (Vienna, 1917), sec. 1. For similar views see also: Franz Brentano, *Psychologie vom empirischen Standpunkt* (Leipzig, 1924), ch. II, sec. 2; Bertrand Russell, *An Inquiry into Meaning and Truth* (New York, 1940), chs. 9–11; Ledger Wood, *The Analysis of Knowledge* (Princeton, 1941), ch. 5; and C. J. Ducasse, "Propositions, Truth, and the Ultimate Criterion of Truth," *Philosophy and Phenomenological Research*, 4 (1944): 317–340.

is mortal is self-presenting to me, does it follow that your believing that Socrates is mortal is self-presenting to you? Without attempting to answer such questions, we can now say something about rules of evidence.

I suggest that when we come to formulate our rules of evidence for empirical (nonlogical) statements, we should first attempt to formulate the conditions under which statements are *directly justified.* Then we should attempt to formulate the conditions under which directly justified statements may be said to *confer evidence* upon still other statements. A possible example of such a rule of evidence would be: "If 'I take something to be red' is directly justified for me, then 'Something is red' is *evident* for me." It may be, however, that this particular rule is more liberal than it should be and that it ought to be replaced by the following: "If 'I take something to be red' is directly justified for me, then 'Something is red' is *initially credible* for me." In the former case, we could say that the directly justified statement confers evidence, in the latter case that it confers only initial credibility. If we take the second of these courses, we might then investigate whether there are certain formal relations which, if they were to hold among a set of initially credible statements, would entitle us to say that the set of initially credible statements confers evidence upon certain other statements.[10] And we may find it necessary to choose between saying (*a*) that some evident statements may be false and saying (*b*) that the only statements that are evident are statements describing those subjective states I have called "self-presenting."[11]

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A NOTE ON JUSTIFICATION*

MANY philosophers describe science as objective and insist that scientists justify their decisions by following the rules of science. The task of the philosopher is then described as one

[10] In *Perceiving: A Philosophical Study* (Ithaca, 1957), I suggested, in effect, the first of these two courses. Something like the second is suggested by Nelson Goodman in "Sense and Certainty," *Philosophical Review,* **61** (1952): 160–167; and by Israel Scheffler in "On Justification and Commitment," this JOURNAL, **51** (1954): 180–190.

[11] I am indebted to students at Brown and Princeton for criticisms of earlier versions of what has been said here.

* Abstract of a paper to be presented in a symposium on "Justification" at the fifty-eighth annual meeting of the American Philosophical Association, Eastern Division, December 29, 1961; this paper will be printed in full in an early 1962 issue of this JOURNAL.

of discovering, systematizing, and possibly vindicating these rules. But a study of the scientist at work reveals that there are (1) some rules that are almost definitions of science, (2) many areas of science that are not rule-covered, and (3) some areas about which no clear decision can be reached. I note that, even in those cases which are rule-covered, the scientist is frequently excused for not obeying the rules, a circumstance which has been slightly misinterpreted by some pragmatists.

Hence the philosopher cannot restrict himself to discovering the rule, but frequently presents and defends rules for science to follow. If we accept the rules proposed by the philosopher, we then will think the scientist is justified when he obeys these rules. Rules of type 1 considered above put bounds on the philosopher, however.

The question then shifts to the justification of the rules proposed by the philosopher and the possible mode of justifying them. No general answer is available. It depends on the type of rule. I then distinguish four types: (*a*) conceptual, (*b*) operational, (*c*) directives, (*d*) explanatory, and discuss some specific difficulties with each. In particular, I review some problems connected with the justification of a simple inductive acceptance-rule, and try to show that its justification depends upon (1) a consistency requirement, (2) beliefs about the aims of science, and (3) our psychological assumptions about human powers. The justification usually sought is of type 2. Philosophers try to show that acceptance of simple inductive rules can be justified if we can give some reason for believing that acceptance of hypotheses in accord with these rules will enable us to predict correctly. A justification of this sort presupposes that the aim of science is prediction, which is not, I think, tenable. But even if prediction is considered one aim of science, a justification of induction along the lines given might be a defeat, not a triumph, for science. If there is justification, it must be sought, I think, in area 3—that is, in terms of our psychological assumptions. Here justification of inductive rules and the explanation of the habit of induction merge.

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COLUMBIA UNIVERSITY

AMERICAN PHILOSOPHICAL ASSOCIATION

EASTERN DIVISION

FIFTY-EIGHTH ANNUAL MEETING

CHALFONTE–HADDON HALL HOTEL,* ATLANTIC CITY,
DEC. 27–29, 1961

PROGRAM

WEDNESDAY, DECEMBER 27

9:00 A.M. REGISTRATION. *Office floor.*

PLACEMENT COMMITTEE OFFICE: *Card Room, office floor.*

11:00 A.M. EXECUTIVE COMMITTEE MEETING. *Bakewell Room, first floor.*

2:00 P.M. FIRST SESSION

SECTION A. Symposium: POLITICAL OBLIGATION AND CIVIL DISOBEDIENCE. *Viking Room, tower floor.*

Richard A. Wasserstrom: "Disobeying the Law"

Hugo A. Bedau: "On Civil Disobedience"

Stuart M. Brown, Jr.: "Civil Disobedience"

Chairman and commentator: Glenn R. Morrow

SECTION B. Symposium: MODAL LOGIC (Joint session with the Association for Symbolic Logic). *West Room, tower floor.*

Alan Ross Anderson and Nuel Belnap, Jr.: "Enthymemes"

Henry Hiż: "Modalities and Extended Systems"

Chairman and commentator: Arthur F. Smullyan

SECTION C. PHILOSOPHICAL PSYCHOLOGY. *Mandarin Room, tower floor.*

2:00 John Silber: "Acts of Volition"
Commentator: Elmer Sprague

2:50 Gerald E. Myers: "Motives and Wants
Commentator: Zeno Vendler

3:40 Daniel D. O'Connor: "Consciousness and the Body"
Commentator: John Fisher

Chairman: Wilfrid Sellars

SECTION D. LANGUAGE, EXPERIENCE, AND ACTION. *Tower Room, tower floor.*

2:00 Lewis White Beck: "Agent, Actor, Spectator, and Critic"
Commentator: Ruth B. Marcus

* All meeting rooms are in Haddon Hall, unless Chalfonte is specifically mentioned.

2:50 Virgil Aldrich: "Too Obvious for Words"
Commentator: Jerome Shaffer
3:40 C. D. Rollins: "Contingent Privacy that Is Complete"
Commentator: Martin Lean
Chairman: Alice Ambrose Lazerowitz

8:00 P.M. SMOKER. *Vernon and Garden Rooms, lounge floor.*

THURSDAY, DECEMBER 28

9:10 A.M. SECOND SESSION

SECTION A. Symposium: DECISION THEORY AND ITS PHILOSOPHICAL SIGNIFICANCE. *Viking Room, tower floor.*
Patrick Suppes: "The Philosophical Relevance of Decision Theory"
Isaac Levi: "Decision Theory and Confirmation"
Chairman and commentator: Hugues Leblanc

SECTION B. Symposium: MENTAL EVENTS. *Rutland Room, first floor.*
Richard Taylor: "The Stream of Thoughts versus Mental Acts"
Keith Lehrer: "Psychological States and Agency"
Chairman and commentator: D. J. O'Connor

SECTION C. LOGIC. *West Room, tower floor.*
9:10 Storrs McCall: "Contrariety"
Commentator: Stephen Barker
10:00 George M. Van Sant: "Breaking Logical Laws"
Commentator: Thomas E. Patton
Chairman: John W. Blyth

SECTION D. ORIENTAL PHILOSOPHY. *Mandarin Room, tower floor.*
Kurt F. Leidecker: "On the Classification of Indian *Darśanas*"
J. A. B. van Buitenen: "Knowledge in Indian Philosophy"
Chairman and commentator: George B. Burch

SECTION E. PHILOSOPHY OF LITERATURE. *Tower Room, tower floor.*
9:10 J. G. Brennan: "Is There a Philosophy of Literature?"
Commentator: Houston Peterson
10:00 George A. Clark: "Proper Names in Logic and Literature"
Commentator: Gertrude Ezorsky
Chairman: Edward G. Ballard

11:00 A.M. BUSINESS MEETING. *Viking Room, tower floor.*

2:00 P.M. THIRD SESSION

SECTION A. IN COMMEMORATION OF ALFRED NORTH WHITEHEAD (1861–1947). *Viking Room, tower floor.*

Robert Palter: "Whitehead's Theory of Relativity: Its Scientific and Philosophic Implications"

A. H. Johnson: "Some Aspects of Whitehead's Social Philosophy"

William A. Christian: "The Concept of God as a Derivative Notion"

Chairman and commentator: Charles Hartshorne

SECTION B. ETHICS. *West Room, tower floor.*

2:00 Paul W. Kurtz: "Decision Making and Ethical Naturalism"

Commentator: Charles A. Baylis

2:50 John W. Lenz: "Universalizability as a Criterion of Moral Prescriptions"

Commentator: Elizabeth L. Beardsley

3:40 Joel Feinberg: "Problematic Responsibility in Law and Morals"

Commentator: Paul Welsh

Chairman: John Hospers

SECTION C. THEORY OF KNOWLEDGE. *Tower Room, tower floor*

2:00 C. J. Ducasse: "Is 'Knowing *That*' Reducible to 'Knowing *How*'?"

Commentator: Vere C. Chappell

2:50 Colin M. Turbayne, "The Linguistic Model versus the Camera Model in Vision"

Commentator: Willis Doney

3:40 Ramon M. Lemos: "Ideas, Images, and Sensations"

Commentator: Theodore T. Lafferty

Chairman: Roderick Firth

SECTION D. PHILOSOPHY OF LANGUAGE. *Mandarin Room, tower floor.*

2:00 Norman Kretzmann: "Significance, Syntax, and Supposition"

Commentator: Paul Ziff

2:50 Lawrence Resnick: "Are Concepts Useful?"

Commentator: K. D. Irani

3:40 Richard M. Gale: "Is It Now Now?"

Commentator: Jack Kaminsky

Chairman: Morris Lazerowitz

4:30 P.M. TEA. *Lounge, Chalfonte.*
7:00 P.M. BANQUET. *Vernon Room, lounge floor.*
Presidential Address by Carl G. Hempel: "Rational Action"

FRIDAY, DECEMBER 29

9:10 A.M. FOURTH SESSION

SECTION A. Symposium: JUSTIFICATION. *Viking Room, tower floor.*

Joseph Ullian: "Luck, License, & Lingo"
Roderick Chisholm: "Rationality and Direct Justification"
Sidney Morgenbesser: "A Note on Justification in the Sciences"

Chairman: Richard B. Brandt
Commentators: Nelson Goodman and Ernest Nagel

SECTION B. EXISTENTIALIST ETHICS. *West Room, tower floor.*

9:10 Richard B. Angell: "Logic and Existentialist Ethics"
Commentator: Luther Binkley

10:00 Louis H. Mackey: "The Loss of the World in Kierkegaard's Ethics"
Commentator: José Ferrater Mora

10:50 Richard Schmitt: "Kierkegaard's Ethics and Its Teleological Suspension"
Commentator: Wilfrid Desan

Chairman: Paul Holmer

SECTION C. HISTORY OF PHILOSOPHY. *Tower Room, tower floor.*

9:10 Max Rieser: "Aesthetic Realism"
Commentator: Robert Rosthal

10:00 Stanley H. Rosen: "Thought and Touch: A Note on the *De Anima*"
Commentator: James F. O'Brien

10:50 David Platt: "Value, Reality, and Some Metaphysical Presuppositions of Theistic Proof"
Commentator: William L. Reese

Chairman: Raphael Demos

2:00 P.M. FIFTH SESSION

SECTION A. Symposium: PHENOMENOLOGY. *Viking Room, tower floor.*

Aron Gurwitsch: "The Problem of Existence in Constitutive Phenomenology"
Quentin Lauer: "Questioning the Phenomenologists"

Rollo May: "Phenomenology and the Theory of the Unconscious"

Chairman and commentator: Marvin Farber

SECTION B. AESTHETICS. *Mandarin Room, tower floor.*

2:00 Milton C. Nahm: "The Relation of Aesthetics to Art Criticism"

Commentator: Theodore Mischel

2:50 Sidney Axinn: "*And Yet:* A Kantian Analysis of Aesthetic Interest"

Commentator: Anna-Teresa Tymieniecka

3:40 Jeanne Wacker: "Intention in Art Criticism"

Commentator: Kingsley Price

Chairman: Barrows Dunham

SECTION C. WITTGENSTEIN'S PHILOSOPHICAL METHOD. *West Room, tower floor.*

2:00 Bernard Gert: "Wittgenstein and Logical Positivism"

2:50 Robert R. Ammerman: "Wittgenstein's Later Methods"

3:40 Douglas Berggren: "Language Games and Symbolic Forms"

Chairman and commentator: A. J. Ayer

SECTION D. HISTORY OF PHILOSOPHY. *Tower Room, tower floor.*

2:00 George L. Kline: "Ethical Theory in Early Russian Marxism"

Commentator: Mary-Barbara Zeldin

2:50 Bernard Baumrin: "'Without the Mind' in Berkeley"

Commentator: D. C. Yalden-Thomson

3:40 Stanley G. French: "Hume's Hurdle"

Commentator: Charles Fritz

Chairman: Leroy E. Loemker

GROUP MEETINGS

WEDNESDAY, DECEMBER 27

SOCIETY FOR CREATIVE ETHICS. *Viking Room, tower floor.*

9:30 A.M. John W. Davis: "Is Philosophy a Therapy or a Sickness?"

Commentators: Thomas R. Bennett, William A. Christian, Charles W. Kegley, Herbert J. Murray, Jr., John Herman Randall, Jr.

Chairman: William S. Minor

12:30 P.M. LUNCHEON MEETING.

PEIRCE SOCIETY. *West Room, tower floor.*

11:00 A.M. Richard S. Robbin: "Critical Common-sensism"
Donald D. Roberts: "Existential Graphs"

SOCIETY FOR ANCIENT GREEK PHILOSOPHY. *St. Giles Room, lounge floor, Chalfonte.*

5:00 P.M. Harald T. H. Reiche: "Xenophanes and the Axiomatization of the God Concept"
Audrey N. M. Rich: "Body and Soul in the Philosophy of Plotinus"

ASSOCIATION FOR REALISTIC PHILOSOPHY. *Viking Room, tower floor.*

7:00 P.M. John W. Lenz: "The New Naturalism in Ethics"

PERSONALISTIC DISCUSSION GROUP. *West Room, tower floor.*

7:00 P.M. Peter A. Bertocci: "In Defense of Metaphysical Creation"

Chairman: Richard M. Millard
Commentator: George B. Burch

JOINT MEETING WITH THE ASSOCIATION FOR SYMBOLIC LOGIC, WEDNESDAY, DECEMBER 27. *Garden Room, lounge floor.*

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